

LETTERS TO THE EDITOR

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[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

Radiant Light and Heat

I AM sure that all students must be grateful to Prof. Balfour Stewart for his exposition in last week's NATURE (p. 322) of the errors and absurdities into which recent scientific men had fallen, and out of which they are now groping their way. But if it be not trespassing too much on his good nature, may I ask him one or two questions in order to further educe his views on points which he cannot but have given much thought to, though they are points which, without further explanation, some of us are liable to misunderstand. We have some of us had the "advantage of being wrong first," combined with the further advantage of thinking ourselves right, but I for one will now gladly admit that I was wrong, if I may thereby hope to join "the generation which is right."

The following are the five points I wish to receive help in understanding:—

(a) "It is absurd to suppose that particles of air are shot . . . with a constant velocity of 1100 feet a second."

I am disposed to agree; but am unable to see clearly how far this absurdity destroys the validity of the so-called "kinetic theory of gases," and of the mode in which sound is considered to be conveyed by such a medium, if indeed it is still so considered at all.

(b) "Can it be thought that hot bodies emit myriads of very small particles, which pass through space with the enormous velocity of 187,000 miles per second? Or again, is it likely that this velocity should be precisely the same for all bodies and for all temperatures?"

I should say it was highly unlikely, in fact, that the idea is ludicrously absurd. This is a triumphant refutation of the corpuscular theory, but I am rather troubled by the thought that the argument seems equally to refute the wave-theory, if for "particles" in the above sentence, we substitute the word "waves." I know it is only my stupidity which causes me to feel this difficulty.

Again, it sometimes seems to me that the undulatory theory itself requires a good deal of "propping up;" and that several phenomena—for instance, "aberration"—explain themselves more easily and simply on the corpuscular.

(c) In speaking of the "transmutation of visible energy into heat," we are surely justified in calling heat "invisible energy" in contradistinction to the other; but, suppose the blow is so intense as to make a flash, are we to consider that flash as part of the invisible energy which has been "created," or are we to consider it a portion of the visible energy which has escaped destruction? The notion of a certain quantity of visible energy disappearing from the universe at one place, and an equivalent quantity of invisible energy being simultaneously created at another, is so beautifully simple and satisfying that I am sure the process can be made quite clear to any mind of common intelligence with a little more trouble.

(d) "This train of thought enables us . . . to assert that there is a definite mechanical relation between the amount of heat which leaves a hot body as it cools, and the radiant energy which accompanies the act of cooling."

I fear I am too stupid to understand this sentence. As I read it, it sounds like the following:—"There is a definite mechanical relation between the number of people which leave a train as it empties, and the number of people who get out of it and go away during the act of emptying." And the paragraph seems to go on thus:—"If, for instance, ten people get out of a train, and all of them enter an omnibus so as to be entirely absorbed by it, then, while the train has become ten people emptier, the omnibus has gained an equal number and has become ten people fuller."

I know that this is absurd, but I am unable to seize the point properly, and therefore venture to put my difficulty in this plain and outrageous way.

(e "Radiant heat is physically similar to radiant light, the

only difference being that its wave-length is greater, and its refrangibility less, than those of light."

May I ask if it is known how much greater "the wave-length of radiant heat" is than "those of light"? The modern distinction between them is evidently so simple and numerical that it must be possible to definitely draw the line and to specify the exact wave-length which characterises each, or at any rate which partitions the one from the other.

Similarly it would be a help to us students to have the refrangibility of radiant heat specified and distinguished from those of light, too.

There are one or two other matters concerning which I should have been glad of further information; but I will not now trespass further upon your space or upon the good nature of the professor.

A STUDENT

IN reply to the remarks of a student I may state as follows:—

(a) In the kinetic theory of gases the pressure of a gas is regarded as being due to a bombardment by the molecules of the gas, and the velocity of sound in any gas can by this theory be shown to be definitely related to the velocity with which these molecules move about.

(b) It is no doubt true that the demonstration of "aberration" on the corpuscular theory of light is of a simpler nature than its demonstration on the undulatory theory, but I have yet to learn that the geometrical simplicity of a demonstration is always a characteristic of truth. The question is rather, Can "aberration" be shown to be a legitimate consequence of the theory of undulations quite apart from the mathematical difficulty or easiness of demonstration? If the demonstration is valid its easiness can wait.

(c) While admitting that our nomenclature regarding energy is of a temporary nature, I have hitherto confined the term "invisible energy" to that kind of energy the motions constituting which are on so small a scale and so rapid that they cannot by any means be rendered visible. No doubt we see a red-hot body, but we do not and cannot see the motions of the individual molecules of the hot body.

(d) The train of thought referred to was that which concluded that the particles of a hot body (like those of a sounding body) are in a state of vibration and (in both cases) communicate their energy of vibration to a medium which surrounds them. It is thus a question regarding energy, therefore a mechanical question, and we are thus entitled to assert that there is a definite mechanical relation of equivalence in energy between the amount of absorbed heat which leaves a hot body as it cools and the radiant energy which accompanies the act of cooling.

We have now so clear and definite a conception regarding energy that "A Student's" simile of a train and an omnibus represents the truth, and it may perhaps look a trifle ridiculous to assert such an obvious equivalence. But my remarks were partly historical, and to the physical student of a past generation the equivalence would not be equally clear. The meaning is that the radiant heat and light given out by a body when cooling, measured in any way you like and used up in any way you like, will always be mechanically equivalent to the amount of ordinary heat which the body has lost.

(e) Your correspondent asks how much greater the wave-length of radiant heat is than that of light. Let me refer him to a diagram which was given in a recent number of NATURE in illustration of a lecture by Prof. Langley, and which will likewise be reproduced in the course of this present series of articles.

BALFOUR STEWART

Pulsation in the Veins

THE writer of a very long and exhaustive article on "The Heart," occupying forty-one pages in Rees's "Cyclopædia," quotes, among other authorities, Bichat, who says "that the blood, when it has arrived at the veins, is no longer influenced by the heart's action; consequently these vessels have no pulsation" . . . "that the blood's return in the veins is involved in an obscurity;" and he propounds as a "contrast" "the fact of general pulsation in the arteries, the absence of this in the veins." The writer of the article states that "many authors, particularly Haller, considering that this [the venous] system has no agent of propulsion, have ascribed to the veins some peculiar structure" of which the evidence is insufficient; also "that there is no analogy to the course of the blood in the

arteries where the action of the heart produces the whole effect," and adds: "There is much obscurity on the subject, as well as in the course of the blood in the general veins; and every judicious mind cannot fail to observe that there is a great vacuum to be filled up."

The object of this paper is to contribute to the solution of this mystery—first by proving that there *is* pulsation in the veins, and that therefore the heart's action *is* directly concerned in the return of the venous blood, and secondly, by suggesting the mode in which it is exerted.

I had observed that, on very close and careful inspection, there was in the veins (in those at the back of the hand, for instance) a visible though exceedingly delicate pulsatory undulation; but so minute that I have generally (but not always) failed to show it to others. It therefore became desirable to devise some means by which the fact might be made more manifest.

In the first instance I requested a lady, who was unable to perceive any pulsatory movement in the veins on the back of her hand, to feel and silently count her own pulse while I counted aloud the beats as indicated by what I could perceive in those veins. She was surprised to find that my counting corresponded exactly with hers, but observed that the beats of her pulse came immediately and *alternately* with my counting. This I had not thought of or suggested. But it is, in fact, what would be the necessary result of the heart's action, and corresponds with its *alternate* contraction and dilatation.

In order to make more evident this venal pulsation the following experiment was successfully tried:—A small piece of silvered glass (about $\frac{3}{8}$ inch by $\frac{1}{4}$ inch) was made to adhere to the surface of a swollen vein on the hand in such manner that *one* edge of the glass rested on the central ridge of the vein, while the other was in contact with the surface clear of the visible vein by the side of it. This, applied in the sunshine, of course afforded a spot of light, the movement of which reflected from the mirror, would indicate the slightest tilting of its plane by the undulating action of the vein, and the result was beautifully conclusive. The light spot vibrated in accordance with the pulse, and its vibrations were in the direction which corresponded with the tilting which should be the result of the position of the mirror in relation to the vein. Then a second mirror was applied on the *opposite* side of the vein, and the vibrations of the light spot took an *opposite* direction, which was to be expected, as the result of a tilting antichinal to the first. A mirror placed on the knuckle (where are no veins of sensible importance) showed no sensible vibration. Thus the objection which I anticipated, that the vibration of the light spot might be caused by the general response in the limbs to the ordinary arterial pulsation, is answered by the evidence that a varied position of the mirror in respect of an individual vein was productive of a correspondingly varied motion of the light spot.

If, then, it is proved that, notwithstanding all previous authorities (to which I have had access), pulsation in the veins does exist, pulsation corresponding in rhythm with that of the arterial system, it becomes a corollary that the heart's action *does* extend to the motion of the blood in the veins, and an evident solution of the mystery of the return of the blood from the extremities appears to result. The expulsive effect of the heart's contraction is familiar, but the effect of its expansion, much in consequence of the venous pulsation having been unseen and denied, has been, as far as I know, ignored. Every one knows how an indiarubber ball syringe is filled by its expansion after compression. Apply this analogy to the expansion of the heart, and the return of the venous blood, the valves in the veins cooperating, would be equally certain. But this involves the existence of a corresponding venal pulsation, the supposed absence of which supported the theory that the direct action of the heart was limited to the arterial system.

I add some directions for the successful trial of the mirror experiment. The pulsatory motion is very small, and the action of neighbouring veins seems to cause parts of the surface to be neutral in respect of the displacement of the plane of the mirrors. It is, therefore, desirable to search experimentally for the best place for them; that is, where the resulting displacement becomes most evident; also the use of some sort of vigorous movement of the body or limbs, such as would cause a general exaggeration of the heart's action, naturally causes the vibrations of the light spot to become more conspicuous. The hand should be supported in the most steady manner, otherwise the pulsatory

vibration becomes mixed up with an indefinite movement of the light spot due to general unsteadiness. The mirrors should not be more than $\frac{3}{8}$ inch square or thereabouts. The silvering is liable to be detached from the glass by adhesion to the skin, if the glutinous substance is applied directly to the back of the mirror; to prevent this its back and edges should be covered with thin gummed paper (such as the margin of a sheet of postage stamps affords). This protects the silvering, so that the mirrors may be used repeatedly, and their position changed as often as may be required; whereas without this precaution they may be spoilt on the first application. Any sticky glutinous substance which does not dry readily (such as indiarubber dissolved in mineral naphtha) is convenient, because by its use the mirror may be with the least trouble shifted from one spot to another, in the search for a place where the venous pulsation is most visibly effective; and this will not always be found exactly where from the appearance of the veins it might be expected to occur.

J. HIPPISELY

Stoneaston Park, Bath, August 10

The Fauna of the Sea Shore

IN the recent correspondence in NATURE on the "fauna of the sea shore," an ambiguity has arisen in the use of the term "littoral."

I, following Prof. Moseley, on whose lecture I was commenting, used the word in the extended sense of describing areas and faunas that were neither "deep-sea" (in the modern acceptance of the term) nor "pelagic." Mr. Hughes, on the other hand, has employed it in its common acceptance as descriptive of the shore area between tide marks.

The portion of the sea-bottom disturbed by waves has at present no term told off to describe it. It is not necessarily "littoral" in any sense, as that word will not cover the case of sand-banks far from the coast; such, for instance, as the banks of Newfoundland, where, according to both zoological and nautical evidence the waves act strongly on the sea-bottom. Some such term as "undal zone" might be used to describe those marine areas where the waves can sensibly affect the fauna.

The downward limit of this undal zone has not, I believe, been hitherto defined. In the case of oscillating waves (the ordinary ocean waves) 50 fathoms seems to approach the practical limit of disturbance, but, according to the evidence of marine charts, the waves appear to make themselves felt at greater depths.

In the late Mr. R. A. C. Godwin-Austen's map of the English Channel (*Q. J. G. S.*, vol. vi. p. 96), the following deposits are indicated, viz.:—

- 40 to 50 fathoms, fine granite shingle with fragments of *Haliotis tuberculata*.
- 50 to 60 fathoms fine granite shingle with fragments of *Patella vulgata*.
- 70 to 80 fathoms, coarse sand and gravel, with decayed *Patella vulgata*.
- 90 to 100 fathoms, coarse sand, fine gravel, *Cardium edule*, *Turbo littoralis*, and *Patella vulgata*.
- Outside 100 fathoms, very fine shell sand, *Pecten varius*, *Cardium edule*, *Patella vulgata*, and *Turbo littoreus*.

Referring to one of these collections of shells (in upwards of 90 fathoms) between Ushant and the Little Sole Bank, the distinguished author remarks:—"Taking the two phenomena together, the occurrence of littoral shells and of marginal shingle, we may safely infer that we have at this place the indication of a coast line of no very distant geological period, buried under a great depth of water, and removed to a great distance from the nearest present coast-line."

The fact that shells are perishable owing to decay, corrosion, and the ravages of marine organisms, seems to me to militate against the probability that the shells in question are of geological antiquity; and their occurrence in connection with sand and shingle instead of mud would rather indicate the present action of currents strong enough to keep the sea-floor clean. This ordinary tidal currents cannot do, though wave and tidal currents combined can.

Under the joint influence of storm waves and storm-engendered currents, light shells may well travel down the channel bed to 40 or 50 fathom soundings. Theory and observation agree in the efficacy of wave and current to this extent. But to account